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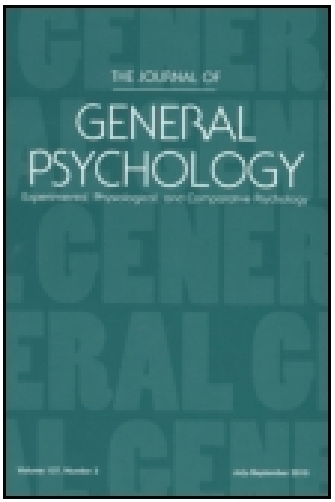

The Journal of General Psychology

Publication details, including instructions for authors and subscription information:
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A Failure to Obtain "Disinhibition"

B. F. Skinner [a]

[a] Biological Laboratories, Harvard University

Published online: 06 Jul 2010.

To cite this article: B. F. Skinner (1936) A Failure to Obtain "Disinhibition", The Journal of General Psychology, 14:1, 127-135, DOI: 10.1080/00221309.1936.9713142

To link to this article: http://dx.doi.org/10.1080/00221309.1936.9713142

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A FAILURE TO OBTAIN "DISINHIBITION"*

From the Biological Laboratories, Harvard University

B. F. SKINNER[1]

The term "inhibition" has been used by Pavlov and his students to refer to apparently any decrease in the strength of a conditioned reflex or to the resulting diminished state. That this is a very broad definition may be shown by listing some of the distinguishable cases that it will include.

1. A reflex may be weakened or wholly suppressed through the prepotent activity of another reflex utilizing the same effectors at least in part. Algebraic summation with a response which utilizes an effector in the opposite way may result in reducing the size of the response, but the strength of the reflex is not reduced, and this is not properly a case of inhibition of this type.

2. A change in the strength of a reflex may follow the administration of an extraneous stimulus that does not elicit a response utilizing the same effectors. (*a*) If the strength increases, the effect is called facilitation (Exner). (*b*) If it decreases, it is called inhibition (Setschenow). Under this restricted definition the processes of facilitation and inhibition are identical except for sign. Case (*b*) cannot here be confused with algebraic summation, and it has a right to the term inhibition on historical grounds, if any restricted use of the term is to be recommended. It is the "external inhibition" of Pavlov.

3. The strength of a reflex, particularly of a reflex characteristic of the intact organism, varies with the drive upon which the reflex is based, where a drive is defined as a variable, the value of which is modified through experimental operations peculiar to drive (4). In the case of hunger, for example, the variable is affected by feeding and fasting, by certain drugs, and by other factors. Strictly speaking, no term corresponding to "drive" is needed. The

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office December 28, 1934.

[1]Society of Fellows, Harvard University.

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observed fact is that the strength of the reflex varies simply with feeding, fasting, and so on; but a hypothetical middle term is convenient. Changes in the strength may be either positive or negative, but it is only in the latter case that the term inhibition is applied. [See, for example, Anrep (1), where "spontaneous inhibition" apparently refers to changes in hunger.]

4. The strength of a conditioned reflex varies with the degree of conditioning (a function of the number of responses reinforced) or the degree of extinction (a function of the number of responses elicited but not reinforced). Here again, changes in strength may be both positive and negative, but the term inhibition is applied by Pavlov and his associates only in the latter case.

The list could be extended by appeal to other writers; for example, it could include

5. reflex fatigue
6. the refractory phase

on the authority of Sherrington (3). But an exhaustive list is not needed to make the present point. It should be obvious that the use of the single property of the negativity of the change does not lead to the establishment of a significant class of data. The cases in the list are related to each other only through the comparatively unimportant fact that they involve *decreases* in reflex strength; with respect to their other properties they differ widely. While there may be no objection to grouping observations together on the basis of a single property, it must not be assumed that other properties possessed by one case are common to the class. This has been done in the case of inhibition. The original type (Case 2*b*) clearly shows the *suppression* of the activity of a reflex. By extension it has been said that Cases 3 and 4 also involve suppression—that a weakening of the drive through feeding suppresses the activity of the reflexes based upon the drive, and that extinction is the suppression of the state of excitability of a conditioned reflex. This paper is concerned with an experimental effect which is said to show the presence of suppression in the case of extinction.

If we may drop the notion of inhibition for the moment and deal with each type of change separately, a simple approach is to regard the strength of the reflex as in each case a function of an inferred variable the state of which is affected through operations upon the organism peculiar to (and defining) the case. This was done above in

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stating Case 3, for a more detailed statement of which see (4). It may also be extended to Cases 4, 5, and 6. In the case of conditioning and extinction, with which we are here concerned, we require a statement of the relation of the strength of a reflex (or of the inferred variable of which the strength is said to be a function) to the experimental operations of reinforcement and of elicitation without reinforcement, respectively. Experimentally this reduces to a description of how a reflex originally of low strength is strengthened by reinforcement and how through lack of reinforcement its strength returns to its original value. Aside from any experimental test, this formulation is obviously the simpler. The notion of extinction as inhibition not only involves a greater number of terms but leads to many difficulties. For example, reconditioning must be regarded as the removal of inhibition (2, pp. 66-67), and it is hard to avoid the conclusion that original conditioning is the same. It is obviously not an economical conception to regard all possible conditioned reflexes as pre-existent in the organism in a state of suppressed excitability.

On the factual side the best support for Pavlov's view is the phenomenon of disinhibition, in which a reflex in the course of extinction is said to be released from inhibition by an extraneous stimulus. This effect, if true, would invalidate the simple formulation given above. Extinction cannot be the mere exhaustion of the effect of conditioning if the strength of the reflex can be restored by an event which in itself has no reinforcing value. Suppose, for example, that we are observing the extinction of a reflex along the dotted curve in Figure 1, which represents the change in strength in time. If we convert this into a summation curve (solid line), which represents the total amount of activity (say, the total number of elicitations under continuous stimulation) in time, we have an envelope

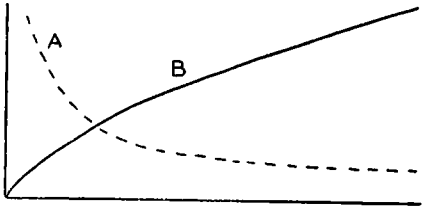

FIGURE 1

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characterizing the variable of which the state is said to be a function. The experimental curve may fall below this envelope if the organism is disturbed in any way, but it should not (except in a minor case to be noted below) go above it. The reality of such an envelope has been repeatedly demonstrated in a large number of cases. Now, the effect reported by Pavlov would be represented schematically as in Figure 2. The presentation of a stimulus at

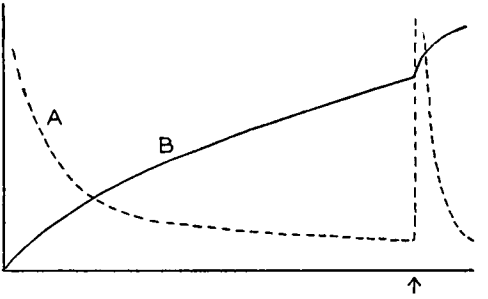

FIGURE 2

the arrow produces an increase in the strength of the reflex. The effect on the summation curve is to send it above its envelope, which is impossible according to the present view.

An experimental attempt to discover disinhibition in typical extinction curves has, however, yielded no result that would violate the present formulation. The method has been described elsewhere (4-8). A white rat presses a small lever, and the movement of the lever may or may not discharge a pellet of food into a tray, where it is accessible to the rat. The rate at which the lever is pressed is recorded. In the typical case of extinction the food magazine is inoperative throughout, and the rate of responding declines in a way that has been described (6-8). In the present experiments extinction curves were used which had been preceded by periodic reconditioning (8), since such curves are not significantly disturbed by the cyclic effect which characterizes curves of original extinction and are therefore better suited to revealing slight changes. The procedure was simply to get extinction curves of this sort in progress and then to introduce the "disinhibiting" stimulus. It may be noted that the method of recording yields a summation curve similar to the

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solid lines in Figures 1 and 2. It would be possible to convert this into the differential form, but, as we have already seen, the summation curve has a special advantage in representing a process of this sort.

Eighteen rats were used in the experiment and two records were taken for six of them, so that 24 cases were obtained in all. Several extraneous stimuli were used. In 12 cases the rats were quickly removed from the apparatus and tossed into the air in such a way that vigorous righting reflexes were evoked. In three cases the tails were pricked lightly with a needle. In other cases the rats were not disturbed bodily. In two, a sound was introduced which was produced by the empty food-magazine. This stimulus might have introduced some slight reconditioning effect; but since any sound would be likely to do this through spread, the present case is preferable as the most clear cut. In seven cases a 3-c.p. light was turned on directly over the lever. No lesser intensities of stimulation were used deliberately, but they have occurred incidentally during many other experiments and are in fact produced inevitably by the rat itself.

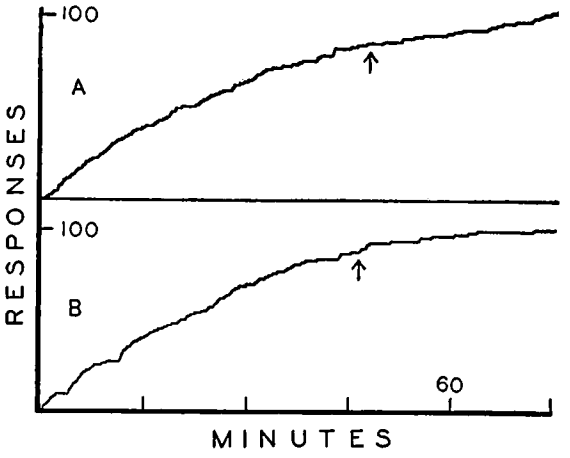

FIGURE 3

EXTINCTION CURVES AFTER PERIODIC RECONDITIONING

At the arrows the rats were quickly removed from the apparatus, tossed into the air, and replaced. Record *A* shows no effect. Record *B* shows one or two (facilitated ?) responses immediately after return, followed by a slight but probably significant depression in rate.

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Since the typical curve obtained with the method is either smooth or falls *below* the envelope, it may be concluded that stimuli of low orders of intensity do not produce the effect we are seeking.

Two typical records are given in Figure 3. At the arrows the rats were removed from the apparatus and tossed into the air as described. The Record *A* shows no significant change, and this was true of six cases. Record *B* shows a very slight increase in rate (perhaps two extra responses) immediately after the return to the apparatus. This was true in one other case with a stimulus of this sort and in four other cases where the stimulus was a light. It was obvious in all these cases, however, that the stimulus was administered at a time when the record was a little below its envelope, and that these extra responses only brought it up to its proper position. We shall return to this point. In all other cases there was not only no increase in rate but an actual decrease after the "disinhibiting" stimulus. The effect may be pronounced and is precisely what we should expect when the stimulus is strong enough to have an emotional effect.

No one of the twenty-four cases gave any result which would invalidate our interpretation of the extinction curve, and the experiments lead us to doubt the reality of the effect reported by Pavlov. They provide several ways of disposing of Pavlov's observations without appealing to a phenomenon of disinhibition. In describing an effect of this sort we obviously need to have the whole extinction curve before us, or at least some considerable part before and after the disinhibition. Otherwise we may mistake certain possible increases in rate in which the stimulus is merely facilitative. This is the possible exception to which we have already referred. It is compatible with the present interpretation and is not an evidence of disinhibition. When the curve is at the envelope the effect will be a momentary increase above this point, followed by full compensation during a later period of decreased rate. So far it has not been possible to produce this effect at will, but examples do occur spontaneously and have been considered (8). Whether or not they invalidate our use of an envelope has also been discussed. Figure 4 shows a possible way of completing Figure 2 in agreement with this view. In the present experiment the light in particular seemed to have a facilitative effect, probably because of its position directly above the lever,

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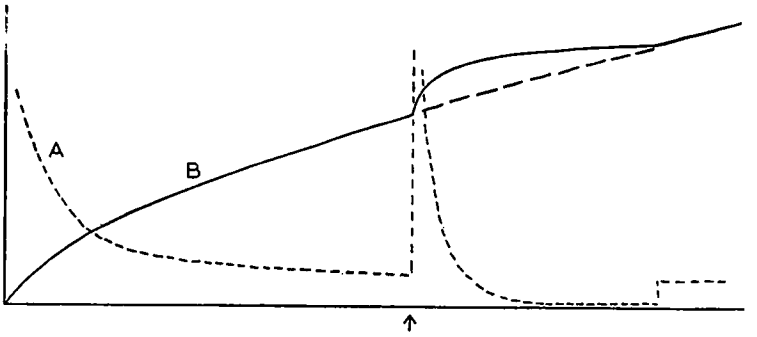

FIGURE 4

by virtue of which it would bring the lever as a source of stimulation more directly before the rat. But, as we have said, the envelope was not exceeded in any of these cases. Whenever facilitation occurred, the curve was for some reason below its envelope, and the effect of the facilitation was simply to bring it to its proper position. The most striking case of this kind is reproduced in Figure 5. The

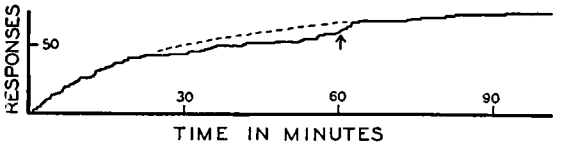

FIGURE 5

EXTINCTION CURVE AFTER PERIODIC RECONDITIONING

At the arrow a light was turned on. The resulting increase in rate is the greatest observed in 24 cases. It is seen to be a recovery from a position considerably below the envelope of the curve.

justification for holding that the rat was considerably "in arrears" is the smoothness of the first part of the curve, which gives it enough weight to keep the extrapolation significantly above the irregular middle section. This can be seen more easily by foreshortening the curve.

Apparently none of the published work on disinhibition gives the curves for extinction upon which the effect was operative. Until this is done and the fact of disinhibition then confirmed, we may

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regard the present interpretation as not seriously threatened. There is, however, one reason why we might expect an apparently positive result in the Pavlov type of experiment which would be lacking in the present case. With the salivary reflex many stimuli may elicit the response. We must deal not only with salivation as part of the ingestion of food and the expulsion of noxious substances but as part of many patterns of emotional excitement and even of investigatory responses. As Pavlov has said, "Footfalls of a paserby, chance conversations in neighboring rooms, slamming of a door or vibration from a passing van, street-cries, even shadows cast through the windows into the room . . . set up a disturbance in the cerebral hemispheres . . ." (2, p. 20). It would be hard to find a disinhibiting stimulus which would not produce salivation of its own accord. If the envelope is exceeded in such a case it may mean simply that other reflexes are being included, that is, that we have a superposition of envelopes. The result is not disinhibition, nor is it embarrassing for the present account.

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Harvard University
Cambridge, Massachusetts

LE MANQUE D'OBTENIR LA DÉSINHIBITION

(Résumé)

Pavlov et d'autres ont employé le terme "inhibition" pour l'appliquer à presque n'importe quelle sorte de décroissance dans la force d'un réflexe et ont supposé que l'effet engage toujours la *suppression* d'un état d'activité. Dans le cas spécial de l'extinction on dit que l'excitabilité d'un réflexe conditionnel est supprimée au moyen d'une élicitation sans renforcement. Une interprétation plus simple est de regarder l'extinction comme l'épuisement de l'état établi dans le conditionnement du réflexe, mais ceci est contredit, paraît-il, par le phénomène de "désinhibition" où un état supprimé se révèle, dit-on.

L'essai-ci de trouver une répétition de l'activité après l'extinction n'a réussi dans aucun de vingt-quatre cas. De divers stimuli "désinhibants" n'ont pas du tout fait élever la force du réflexe ou ont causé une petite quantité d'activité, chose attendue selon l'analyse du reste de la courbe d'extinction. Localement l'effet est en quelques cas semblable à celui rapporté par Pavlov, mais considéré à la lumière de la courbe entière il ne suggère jamais, paraît-il, la suppression de l'excitabilité par le processus de l'extinction. La conception de l'extinction comme simplement l'épuisement de l'effet du conditionnement n'est pas affectée.

SKINNER

ÜBER DAS AUSBLEIBEN DER "ENTHEMMUNG"

(Referat)

Pawlow und andere haben das Wort "Hemmung" auf beinahe jede Art von Abnahme der Stärke eines Reflexes angewandt und angenommen, dass die Wirkung die Folge der Unterdrückung eines Zustandes der Tätigkeit sei. In dem besonderen Fall der Ausschaltung meinen sie, dass die Reizbarkeit eines bedingten Reflexes durch Hevorrufung ohne Verstärkung unterdrückt wird. Eine einfachere Deutung betrachtet die Ausschaltung als die Erschöpfung des Zustandes, der beim Bedingen des Reflexes aufgestellt wird, aber dies wird wahrscheinlich durch das Phänomen der "Enthemmung" widerlegt, in dem ein unterdrückter Zustand sich offenbaren soll.

Der jetztige Versuch, eine Erneuerung der Tätigkeit nach Ausschaltung zu finden, misslang in jedem der vierundzwanzig Fälle. Verschiedene "enthemmende" Reize haben entweder die Stärke des Reflexes überhaupt nicht erhöht oder haben zu einer kleinen Tätigkeit geführt, die von einer Analyse des Rests der Ausschaltungskurve erwartet sein könnte. Örtlich ist die Wirkung in einigen Fällen dem ähnlich, was Pawlow berichtet hat, aber im Lichte der ganzen Kurve betrachtet, deutet es nie auf Unterdrückung der Reizbarkeit durch das Verfahren der Ausschaltung hin. Die Auffassung der Ausschaltung als einfache Erschöpfung der Wirkung des Bedingens wird nicht beeinflusst.

SKINNER

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